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pending the choice of time by the British association for their Montreal meeting. A preference, however, has been indicated for the week beginning Sept. 3, — a date earlier than usual, but welcome to all who know how warm Philadelphia can be in August.

W. C. W.

RELIABILITY OF THE EVIDENCE OBTAINED IN THE STUDY OF CONTAGIA.

THERE is certainly a disposition, among some of our scientific men, to doubt the possibility of making direct and satisfactory demonstrations of the rôle played by the schizophytes, or microbia, in the production of disease, and that which they may be compelled to take in its prevention. Recent publications by accepted authorities have tended rather to confirm these doubts than to remove them, and we are frequently asked if our results are not founded on probabilities rather than on definite and conclusive facts. While this uncertainty is still felt, it is well to occasionally review the connection between the facts established and the conclusions drawn from them.

Though the schizophytes are the smallest of living organisms, that is not an insurmountable obstacle to their careful study, as is proved by the well-known investigations of the *Bacillus anthracis* by Koch. His demonstration that this exists in two forms (a vegetating filament and a spore), and that the latter survives unfavorable conditions which destroy the former, enabled him to trace a connection between the activity of the virus and the life of the parasite, which other investigators had failed to establish. Thus, the blood of anthrax victims, which contained only *Bacillus* rods, lost its power to reproduce the disease after a few days' putrefaction; while that which contained spores remained virulent an indefinite time. A certain degree of cold, and also an insufficient supply of oxygen, prevent the formation of spores; and, the filaments being short-lived, the organism loses its vitality in a few days under such conditions. If spores had formed before the liquid was exposed to these conditions, however, they were unaffected, and were capable of germination after weeks or months. Again: if a virulent liquid was largely diluted, the filaments were destroyed, but the spores survived. In all these cases the activity of the virus disappeared with the death of the organism, and was retained whenever the formation of spores had enabled this to resist the unfavorable conditions.

Here was a proof of the pathogenic character of the schizophyte much more satisfactory than the mere demonstration of its presence in all cases of the disease, or the additional evidence that it might be passed through a certain number of cultivation-flasks; the liquid in the last being as virulent as in the first.

Since Koch's paper was published, Pasteur has added observations of an equally convincing character. The liquid part of the virus may be freed from the organism either by filtering through plaster or by decanting after it has stood in a constant temperature for a few days to allow the germs to gravitate to the bottom of the flask. In either case the liquid is harmless, and the separated germs still produce the disease. Again: compressed oxygen destroys the filaments, but does not affect the spores; and a virus containing only the former loses its activity when treated with this agent, while one in which spores have formed retains its virulence.

We are able to say, therefore, that, in the disease known by the French as charbon and by the English as anthrax, no liquid is virulent unless it contains the living *Bacillus anthracis*, and that the death of this organism always coincides with the destruction of the virulence.

This demonstration of the pathogenic action of the *Bacillus* cannot but be regarded as equally satisfactory with what is obtained by investigations in other departments of biological science. If the observations of these gentlemen are accurate, and they have been confirmed too often to be doubted, then there is no escaping the conclusion that the *Bacillus anthracis* is the essential and only cause of anthrax.

It is not to be denied, however, that the size of the parasite in anthrax, and the fact of its existence under two forms having such unequal resistance to unfavorable conditions, were characters which greatly facilitated the demonstration of its pathogenic relation to the disease. Is it possible to obtain equally satisfactory evidence in regard to the smallest of the schizophytes, and one which only exists in the vegetating condition?

The micrococcus of chicken-cholera is of this kind, and it is consequently very interesting to see just what progress we have made in demonstrating its identity with the virulent principle. We know from Pasteur's investigations that it is always present in this disease; that it may be cultivated, and passed from flask to flask for many times, without losing its virulence. The filtered liquid loses its activity; that from which the germs are

separated by gravitation is equally harmless. Taking up the study here, I have proved that the exact degree of heat which, in a given time, kills the micrococcus (132° F. for 15 minutes), destroys the virulence at precisely the same point; also that the proportion of carboic acid, of sulphuric acid, and of a solution of chlorides (Platt's), which destroys the virulence in from two to four hours, corresponds with the proportion which is required to kill the organism in the same time.

The effect of heat and of these disinfectants on the virus was determined by inoculation experiments. The point at which the micrococcus is killed was learned by placing a drop or two of virus in the sterilized liquid of a cultivation-tube after the proper proportion of disinfectant had been added. In a given time a drop was taken from this tube, and placed in a second one which contained a favorable medium for the growth of the germs. If the schizophytes had been destroyed by the disinfectant, there would be no multiplication; while, if they had resisted it, they would certainly reveal the fact by developing in their usual manner. The exact correspondence which exists between the results of the two series of experiments in every case, is also an evidence of the reliability of the method.

While it might be conceived, that, even though the virulent agent consisted of something entirely different from the micrococcus, both might be destroyed by the same degree of heat in the same time, it is not conceivable that this would also occur from the effect of three different chemical agents. If it were necessary, this line of evidence could probably be increased indefinitely; but it is already equal to what is usually considered necessary to demonstrate a point in other departments of science.

It is possible, then, by present methods of research, to determine satisfactorily whether a given organism is the cause of a certain disease, or whether it is an epi-phenomenon; and, if there is still much doubt in regard to some of these, it would seem to be owing to the fact that observers have relied too implicitly upon the microscope, and neglected the cultivation and inoculation experiments, that are essential to definite and reliable conclusions.

D. E. SALMON.

SPONGE-CULTURE IN FLORIDA.

THE U. S. national museum has lately received from Messrs. McKesson and Robbins,

sponge-importers of New York, an interesting contribution representing the first successful attempts at sponge-cultivation on the American coast. It consists of only four specimens, all of the finest or sheep's-wool variety, which were raised from cuttings at Key West, Fla., by the agent of the above-named firm. The localities in which the sponges were planted were not the most favorable for sponge-development, and their growth was therefore less rapid and perfect than might otherwise have been the case. They were fastened to the bottom, in a depth of two feet and a half, by means of wires or sticks running through them, and allowed to remain down a period of about six months before they were taken up. Fully four months elapsed before they recovered from the injury done them in the cutting, which removes the outer 'skin' along the edges of the section; and the actual growth exhibited was for about two months only. The original height of each of the cuttings was about two inches and a half. One was planted in a cove or bight where there was little or no current, and its increase in size was very slight. The other specimens were placed in tide-ways, and have grown to from four to six times their former bulk, which certainly promises well for the future of sponge-propagation. Two hundred and sixteen specimens in all were planted at the same date, and, at the last accounts, those which remained were doing finely.

The chief obstacle to the artificial cultivation of sponges at Key West arises from the fact that the sponge-fishermen infest every part of the region where sponges are likely to grow, and there is no legal protection for the would-be culturist against intruders. The enactment of judicious laws bearing upon this subject by the state of Florida, or the granting of special privileges conferring the right to occupy certain prescribed areas for sponge-propagation, would undoubtedly tend to increase the annual production of this important fishery, which has remained at a standstill for several years past, mainly because of the partial exhaustion of several of the most extensive sponging-areas.

Accompanying these artificial growths was a collection of over a hundred specimens of the various grades of Florida sponges of different sizes, each labelled with its supposed age, based upon estimates of the average rate of growth, by the sponge-collectors. This entire collection now forms a part of the American exhibit at the great London fisheries exhibition.

R. RATHBUN.